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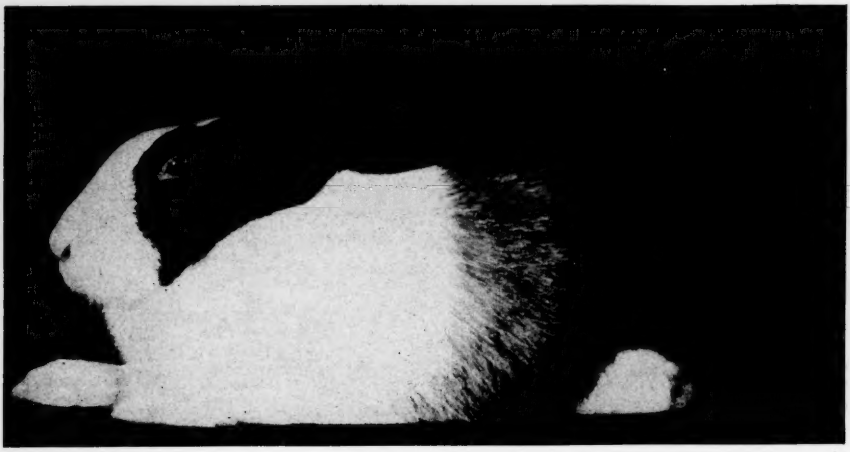

A MOSAIC RABBIT
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A Silver Muskrat

Age and Hair Form

A Map of Chromosome Four

Susceptibility to the Corn Earworm

Spotting Genes in the Lady Beetle

VOLUME 35 NOVEMBER, 1944 NUMBER 11

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An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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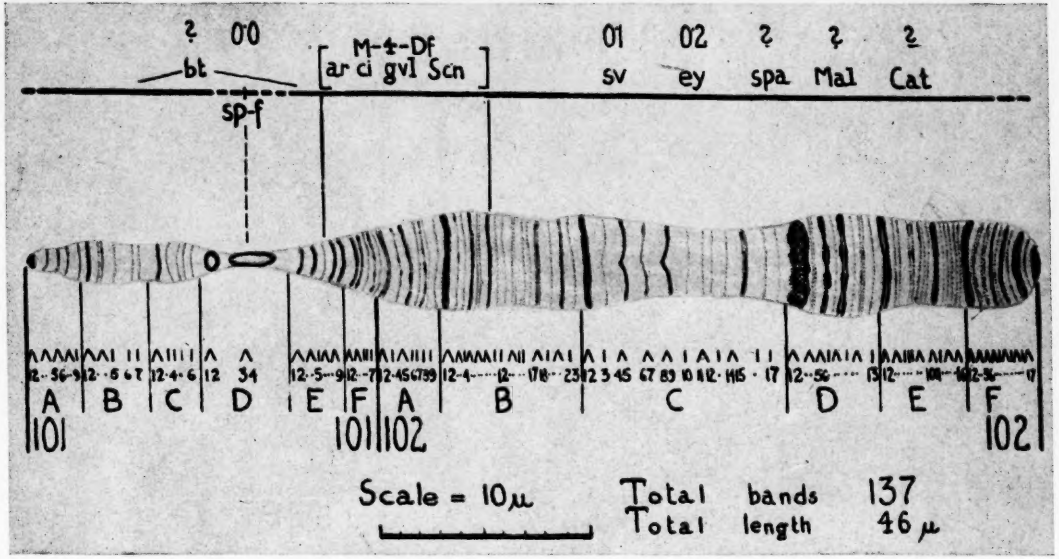


MAP OF CHROMOSOME 4

Figure 1

This map is a composite drawing based on eight preparations. The dimensions here shown represent averages derived from the individual drawings and the individual bands have been fitted in by reference to all of them.

A REVISED MAP OF SALIVARY GLAND CHROMOSOME 4

Of *Drosophila melanogaster*

B. M. SLIZYNSKI

Institute of Animal Genetics, University of Edinburgh, and Department of Biology and Genetics, Polish Medical School, University of Edinburgh

SINCE the importance of salivary gland chromosomes was pointed out by Painter, the emphasis in Drosophila work has shifted from breeding methods to direct analysis of polytene chromosomes.

For this kind of work, standard maps of normal salivary gland chromosomes are necessary. The first complete mapping of the entire chromosome structure in the nuclei of salivary gland cells was done by C. B. Bridges in 1935. These maps were soon followed by revisions (made by C. B. Bridges and later by P. N. Bridges), which included all the large chromosomes of *Drosophila melanogaster,* but omitted the dot-like chromosome 4.

The present author has not made any special and deliberate efforts to revise the map of chromosome 4. During various salivery gland chromosome studies of quite different scope, several excellent figures with an exceedingly well-outstretched chromosome 4 have been noticed. Advantage has been taken of these for the purpose of preparing a more detailed map of this chromosome.

Photomicrographs of three of these preparations are presented in Figure 2. These are at approximately the scale used in the generalized map, as is indicated by the ten-micron scales drawn in each of the photographs. Incidentally, the scale of this map is the same as that used in the series of revised maps prepared by C. B. Bridges and P. N. Bridges.

The revised map (Figure 1) consists of a generalized composite of eight drawings, its dimensions being the average of their lengths and widths, and the individual bands being fitted in by reference to all the drawings.

Comparison of the C. B. Bridges map with the present one shows an increase in the number of lines from 51 to 111 in the right arm of the chromosome, and an increase in length from 15 microns to 32 microns.

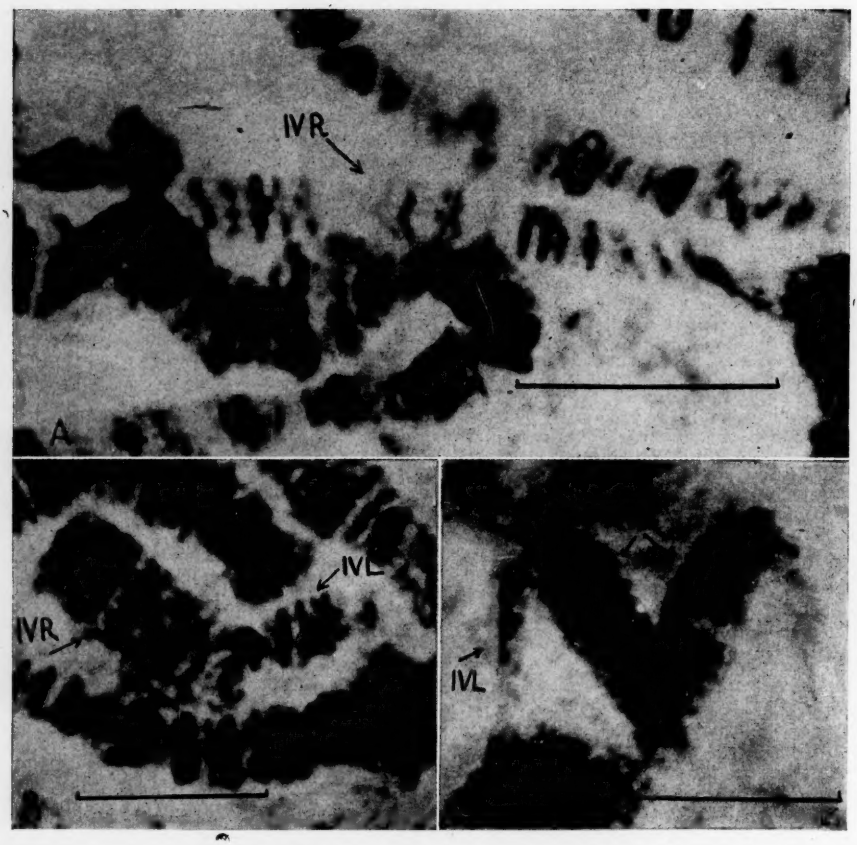
The left arm (4L) of chromosome 4 was first described for *Drosophila melanogaster* by Panshin and Khvostova in 1938, by Griffen and Stone in 1940, and by the present author in 1941.* This revision has revealed minor additions to these earlier maps.

Structure

The bands of chromosome 4 have an especially fine-grained and dotted appearance, almost devoid of the very heavy capsules so conspicuous in the other chromosomes. Usually both ends of 4R are attached to the chromocenter, thus forming a U-shaped structure. This double connection causes the shortening and thickening of the body of the chromosome. With excessive stretching, however, one or other of the ends breaks away and the chromosome becomes more elongated. The left arm of chromosome 4 is more difficult to observe, since on account of its small size it usually becomes embedded in the chromocenter. On the free ends of both arms, as well as in the proximity of the centromere (*sp-f*), there is heterochromatin.

The divisions and subdivisions of C.

*The presence of a short arm was, however, first reported by B. P. Kaufmann (*Jour. Morph.* 56:125-155, 1934) from a study of neuroblast cells of the larva.



PHOTOMICROGRAPHS OF CHROMOSOME 4

Figure 2

Three exceptionally good preparations of the fourth chromosome of *Drosophila melanogaster* showing fine structural details. The scale on each photomicrograph represents ten microns. *A* shows the right arm of chromosome 4 enormously outstretched between two parts of the disrupted chromocenter. *B* shows the two arms of this chromosome completely broken off from the chromocenter, but still connected with each other in the central spindle attachment region. *C* shows the free ends of the arms attached to the chromocenter with their proximal parts broken off. The right arm of the chromosome is partly not clear.

B. Bridges' reference system have been retained without change for 4R, and those of Griffen and Stone for 4L.

So far as it was possible to ascertain it, the comparison of the cytological and genetic maps has been included.

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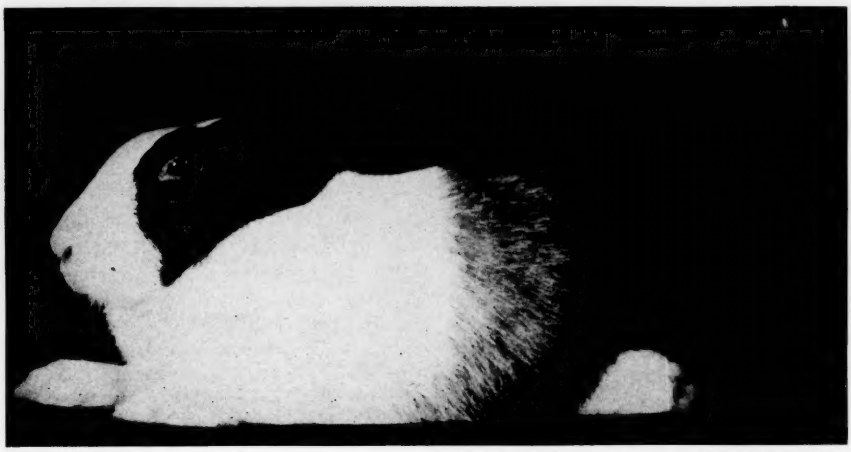
ANOTHER CASE OF A BLACK-BLUE MOSAIC IN THE DUTCH RABBIT

SEDGWICK E. SMITH*



A MOSAIC RABBIT

Figure 3

This is a female Black Dutch rabbit with a semicircular patch of blue hair (dilute black) adjacent to the white belt. The simplest hypothesis to account for this condition depends on the fact that the rabbit is heterozygous for the intensity gene *I*. A somatic mutation to the dilute blue region makes this area *ii* with respect to this gene, and results in the blue patch.

THE work of the U. S. Plant, Soil, and Nutrition Laboratory requires the maintenance of a stock colony of rabbits. Among the offspring of the laboratory stock colony of Dutch rabbits appeared a female of an unusual coat color combination. This rabbit was predominantly a black Dutch with an unusual patch of blue hair adjacent to the white shoulder belt. A survey of the literature disclosed the description of rabbits similarly marked, and this contribution is offered as additional information on this character of genetic interest.

In rabbits, as in many mammals, color (black in this instance) is modified by a pair of allelic factors. The dominant member *I* of this allelic pair conditions an intense color, whereas the recessive member *i* leads to a dilution of the color. In the case of black, *i* when present in the homozygous condition, leads to a blue color. Adequate data exist[2] to show that *I*, when present either in the homozygous or heterozygous state, conditions a coat color uniformly intense.

Pickard[4] described a male rabbit that was a mosaic not only of coat color but also of hair length. This rabbit was mostly black with four patches of brown-tipped hairs in its coat. In addition to this, three patches of long (Angora) hair occurred in a coat predominantly short-haired. Castle[1] reported a coat color mosaic in a black Dutch male rabbit. The unusual color

*U. S. Plant, Soil, and Nutrition Laboratory, Agricultural Research Administration, U. S. Department of Agriculture, Ithaca, New York.

was a patch of blue hair on the left shoulder, apparently confined to the area normally appearing as white in a Dutch rabbit. The interesting feature about this case was that the tricolor condition (black, white, and blue) appeared in three offspring out of a possible 78 which might have shown the character. An F_1 tricolor male sired an additional three tricolors out of a possible 83 offspring which might have shown the condition. Of note was the fact that the blue area of hair regularly appeared adjacent to or in the white belt over the shoulders. Pickard[5] described a very similar mosaic in a male Dutch rabbit. Here again the rabbit was predominantly a black Dutch with a blue area apparently confined to the area normally white in a Dutch rabbit. This male sired 144 offspring, none of which displayed a mosaic pattern.

The mosaic rabbit here reported (Figure 3) was born of black Dutch parents. This mosaic female when bred to her sire produced a total of 14 offspring in three litters, all of which were normally marked. Of these 14 young, 7 were black Dutch, 1 was blue Dutch, and 6 were albinos, showing that the female was heterozygous for the dilution factor *i* and the color factor *c*.

Since color mosaics in rabbits are rare, it is interesting to note that three of the four cases described, including the mosaic offspring of Castle's original mosaic, are of the same type, namely, the presence of an area of blue fur in black Dutch rabbits and furthermore that this blue area occurred regularly either in the area of the normal white belt or adjacent thereto. Castle pointed out the apparent association of the mosaic area and white spotting in his rabbits. Dunn[3] surveyed the known cases (21) of mosaics in rodents and pointed out that with the exception of two or three cases, the mosaic condition was associated with white spotting. The genetics which may be involved in such mosaics has been discussed in the papers of Castle and Dunn. Of the various hypotheses advanced, the simplest one is to assume that the mosaic area is the result of a somatic mutation of *I* to *i* so that the cells of this area have the *ii* genotype which in this instance produces a blue color.

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Buzz-Bombs Over Merton

Excerpts from letter from Dr. P. C. Koller to Dr. H. J. Muller: "You may have heard that on the 25th of August the John Innis was hit by a flying bomb. Cyril (Darlington) and myself just escaped narrowly. The bomb stopped over our house and glided into the Institute, exploded about 20 yards from Cyril's lab. What a mess! The roof was blown off and no windows were left. Some experimental work of the Institute, especially Mather's, is completely lost. Luckily it was a Sunday, and no life was lost."—An earlier letter told of a workman being killed on the grounds of the Institute, and the present letter told of another bomb passing over Darlington's house, but without stopping. It mentions the difficulties of running to shelter under tables with Darlington's three children,—one just born. "We are having a pretty stiff time yet."

MAIZE HYBRIDS SUSCEPTIBLE TO EARWORM

Heritable Differences in Susceptibility of Corn Hybrids to Early Attack

FREDERICK D. RICHEY†

SERIOUS damage from the feeding of corn earworms was observed in certain rows of one of the corn breeding plots at the Tennessee Agricultural Experiment Station in early June of 1944. Most of these rows contained the single cross of two inbred strains, 713 × T61. Inbreds T13 and T61 appeared also in the parentage of hybrids in other rows less seriously affected.

On June 15, 50 days after planting and about 10 days before tasselling, the undamaged and damaged plants were counted. The damaged plants were graded into: (1) those with very slight damage, (2) those with moderate damage, and (3) those with very severe damage. The damage to an average grade-3 plant is shown in Figure 4. Data were taken on 21 rows. The first six comprised the possible hybrid combinations among T13, T14, T18, and T61. In the other 15 rows T13 × T61 alternated with three open-pollinated varieties and five single crosses from other states.

In the nine rows of T13 × T61, 71 per cent of the plants had evident damage, and the mean grade for damaged plants was 2.0. The other 12 of the 21 rows had only 26 per cent of the plants damaged with a mean grade of 1.3 for the damaged plants. Thus, many more of the T13 × T61 plants were damaged

SUPER SUSCEPTIBLE HYBRID

Figure 4

Hybrids between two inbred strains of maize (T13 × T61) were markedly susceptible to the corn earworm. This plant is classified grade 3 in susceptibility. Other hybrids involving these two lines also showed more than usual susceptibility. The reason why progeny of these inbred are singled out for attack is not known.

*Cooperation between the Division of Cereal Crops and Diseases, Bureau of Plant Industry, Soils, and Agricultural Engineering, Agricultural Research Administration, U. S. Dept. of Agriculture, and the Tennessee Agricultural Experiment Station.

†Principal Agronomist.

and the damage was much more serious than the average for the varieties and for other single crosses.

The data for the combinations among the four Tennessee inbreds are shown, by parentage, in Table I. It is obvious that T13 and T61 impart susceptibility to their hybrids. This susceptibility comes to full fruition in the cross T13 × T61. T18, on the other hand, appears definitely resistant and T14 is intermediate. No reason for these differential reactions is apparent. They were not associated with plant height or maturity.

Table I—Percentage of all plants showing damage and mean grade for damaged plants in the six single crosses among four corn inbreds.

	Inbred parent lines							
	T13		T14		T18		T61	
	% a	Grade b	% a	Grade b	% a	Grade b	% a	Grade b
T13	...	...	50	1.6	33	1.8	86	2.6
T14	50	1.6	...	...	31	1.3	69	1.9
T18	33	1.8	31	1.3	...	...	18	1.0
T61	86	2.6	69	1.9	18	1.0	...	...
Mean	56	2.0	50	1.8	27	1.4	57	1.8

a/ Percent of all plants damaged.

b/ Grade of damaged plants: 1, very slight, to 3, very severe.

✦

Social Factors in Expression of Mental Illness

. . . It seems to be useful at this point to differentiate between two concepts: that of defect and that of neurosis. If a person fails to attain freedom, spontaneity, a genuine experience of self, he may be considered to have a severe defect, provided we assume that freedom and spontaneity are the objective goals to be attained by every human being. If such a goal is not attained by the majority of members of any given society, we deal with the phenomenon of *socially patterned defect.* The individual shares it with many others; he is not aware of it as a defect, and his security is not threatened by the experience of being different, of being an outcast, as it were. What he may have lost in richness and in a genuine feeling of happiness is made up by the security of fitting in with the rest of mankind—*as he knows them.* As a matter of fact, his very defect may have been raised to a virtue by his culture and thus give him an enhanced feeling of achievement. . . .

Spinoza has formulated the problem of the socially patterned defect very clearly. He says: "Many people are seized by one and the same affect with great consistency. All his senses are so strongly affected by one object that he believes this object to be present even it if is not. If this happens while the person is awake, the person is believed to be insane. . . . But if the *greedy* person thinks only of money and possessions, the *ambitious* one only of fame, one does not think of them as being insane, but only as annoying; generally one has contempt for them. But *factually* greediness, ambition, and so forth are forms of insanity, although usually one does not think of them as 'illness.'" These words were written a few hundred years ago; they will hold true, although the defect has been culturally patterned to *such* an extent now that it is not generally thought any more to be annoying or contemptuous. Today we come across a person and find that he acts and feels like an automaton; that he never experiences anything which is really his; that he experiences himself entirely as the person he thinks he is supposed to be; that smiles have replaced laughter, meaningless chatter replaced communicative speech; dulled despair has taken the place of genuine pain. Two statements can be made about this person. One is that he suffers from a defect of spontaneity and individuality which may seem incurable. At the same time it may be said that he does not differ essentially from thousands of others who are in the same position. With *most* of them the cultural pattern provided for the defect saves them from the outbreak of neurosis. With *some* the cultural pattern does not function, and the defect appears as a severe neurosis. The fact that in these cases the cultural pattern does not suffice to prevent the outbreak of a manifest neurosis is in most cases to be explained by the particular severity and structure of the individual conflicts. I shall not go into this any further. The point I want to stress is the necessity to proceed from the problem of the *origins of neurosis* to the problem of the *origins of the culturally patterned* defect; to the problem of the *pathology of normalcy.*—ERICH FROMM, in *Amer. Soc. Rev.*, Aug., 1944.

INHERITANCE IN LADY BEETLES

II. The spotless pattern and its modifiers in *Hippodamia convergens* and their frequency in several populations*

A. FRANKLIN SHULL

IF knowledge of the genetics of color pattern in the Coccinellidae is to be used for analysis of natural populations, particularly geographic races, of this family, and through them for the study of evolution, it is desirable that the facts relating to a widely distributed species be obtained early. For the Americas, one species which eminently fulfills this requirement is *Hippodamia convergens* Guer.

The Usual Elytral Pattern

The most common pattern of this species consists of six black spots on each elytron, zigzagging from an anterior outer (humeral) spot back to a spot (the apical one) near the posterior tip (Figure 5*A*). There is often a slight stripe shared by the two elytra at their inner front corners (the scutellar stripe), but this is ignored because the shadow in the crevice between the elytra makes it difficult to judge the size of the black area when this is small.

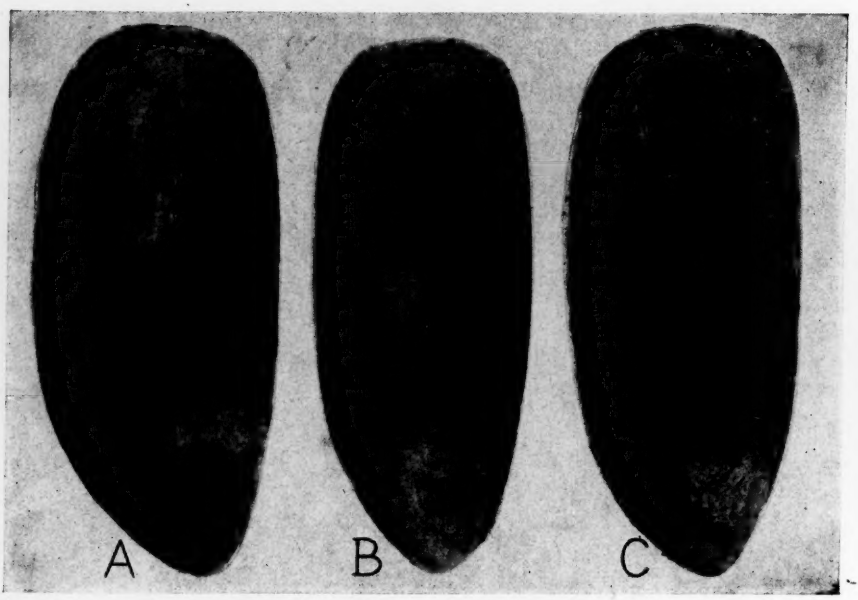
The most striking deviation from this "wild-type" pattern is one which may be called "spotless." The latter variety may be actually spotless (Figure 5*B*), but more often some of the spots may be present, though greatly reduced (*C*). The anterior three spots are more often present in the spotless variety than are the rest, but no one of the six spots is *always* absent. Indeed, all six spots may be present in the same spotless beetle, though most or all of them are much reduced. How to recognize the spotless individuals under these circumstances is discussed later in this paper.

Experiments to test the inheritance of the spotless and spotted pattern were performed with beetles collected in Ann Arbor, Michigan, some obtained from Placerville, California (Mr. E. M. Fields), and, doubtfully, some which were extracted from earlier crosses involving beetles from Placerville, Yosemite Valley (obtained through the courtesy of Supt. Frank A. Kittredge, of Yosemite National Park), and certain other forms. The crosses dealing with these three groups are reported separately in Table I.

The first cross was made with a spotless female and a spotted male collected in Ann Arbor. The female was not assuredly virgin, but if she had mated previously the chance is very great that the male involved was spotted. The parents in this first cross were lost, but are described in the records as "spotless" and "typically 12-spotted." The experiment is listed as No. 20 in the first line of Table I, where it is shown that the offspring were 21 spotless and 12 spotted. Clearly one of the parents was homozygous, and the next five experiments in the table indicate that it was the spotless one. Spotless × spotless (Exps. 37, 38, 39) together yielded 74 spotless and 27 spotted, while spotted × spotted (Exps. 36, 42) yielded only spotted. A further mating of spotted × spotted (Exp. 53), using progeny of Experiment 36, yielded also only spotted. Spotless is plainly dominant, though the question how complete the dominance is must be raised later.

From the Placerville collection two matings were made to obtain virgin females. One was a pair of spotless beetles (Exp. 264), the other a group of four or five spotted ones (Exp. 265). The two spotless parents were clearly both heterozygous, as was to be expected from the infrequency of that type. Between these two progenies eight single-

*Contribution from the Department of Zoology, University of Michigan. Aided by a grant from the Faculty Research Fund of the University of Michigan.



SPOTTED AND SPOTLESS PATTERNS

Figure 5

Left elytra of the lady beetle (*Hippodamia convergens*) showing the normal spotted pattern (*A*) and the spotless pattern (*B* and *C*). In the "spotless" beetle the spots are actually absent more rarely than in the type wherein some of the spots appear (*C*) but are greatly reduced in size. See Pages 331 and 333-334 for a discussion of the limits of variation in this type.

pair crosses were made,—three of one of the reciprocal crosses, five of the other, as shown in the Placerville section of the table. In seven of these eight crosses the spotless beetle chosen must have been homozygous, while in only one (Exp. 273) was the spotless parent heterozygous. Since the heterozygous spotless progeny of Experiment 264 should have been twice as numerous as the homozygous spotless, the preponderance of homozygous ones among those used in crosses suggests that the choice of individuals for parents was not random. If dominance were not quite complete, and if the least spotted individuals were selected as insurance that they were really of the spotless kind, the choice may have fallen more often on the *SS* genotype than on the *Ss*.

The so-called "extracted" beetles referred to in the last section of Table I were the descendants of earlier crosses, as described above. The precise nature of these crosses need not detain us here. The beetles used were sufficiently near to the spotless and spotted varieties found in nature to suggest that the same pair of genes was involved, even if accompanied by minor genes at other loci. Experiment 307, from which the extracted beetles were obtained, was obviously the mating $Ss \times ss$, so that the spotless offspring used in the next four experiments in the table must be heterozygous. Their progeny show them to be such. The spotted beetles from 307 are in like manner shown, in the last four lines of the table, to be homozygous.

Dominance of Spotless

The spotless variety plainly differs from the spotted variety by one gene. How nearly dominant the spotless gene is is not clear from the table, since no details of the spotless pattern are there

given. One observation mentioned above suggests that dominance is not quite complete. Some slight additional evidence on this point is obtainable from the experiments of Table I, for seven spotless parents (Exps. 266 to 272) are there shown to be homozygous, while their 102 offspring must be heterozygous; and six parents (Exps. 273 to 325, as arranged in the table) are demonstrated to be heterozygous, and their 71 spotless offspring must also be heterozygous.

To use this comparison, it is necessary to have some method of rating the extent of the spotting. A rough method, devised early in the experiments and consequently based only on Michigan beetles, was to consider the largest spot yet observed up to that time in each of the six positions as having a value of 4. Smaller spots in each position were rated 3, 2 or 1. The size of spots was ordinarily estimated, but at frequent intervals they were measured, to keep the standards fresh in mind. The darkest beetle possible under this scheme had a rating of $6 \times 4 = 24$. It was later necessary to increase the maximum rating of the spots to 5 or even 6 to accommodate the California beetles, so that presumably a maximum in that region might be $6 \times 6 = 36$. However, relatively few of the beetles in either region attain the maximum in all six spots.

Since the Michigan standard is the better established of the two, and since only relative, not absolute, values are wanted, the experimental beetles referred to above are rated as a percentage of the Michigan maximum, even though all the beetles compared are western derivatives. The seven homozygous spotless parents had a mean rating which is 7.14 per cent of the maximum, while their 102 heterozygous offspring had a mean rating of 9.93 per cent of the maximum. The six heterozygous parents had a mean rating of 12.50 per cent of the maximum, and their 71 heterozygous offspring a mean rating of 13.79 per cent of the maximum. It would be possible to interpret these figures to mean a slight incompleteness of dominance of the spotless pattern, but the difference would be negligible in field studies.

Recognition of Spotless

Since all of the spots may be present in a spotless beetle, the question arises whether there is any overlapping between spotless and spotted. To answer this question the same scheme of measurement described above may be used. However, since it is the anterior three spots which are most often present and relatively the largest in spotless beetles, a sharper contrast between spotless and spotted may be made by considering only the three posterior spots. These are usually the largest ones in a spotted beetle, and relatively the smallest (if present at all) in a spotless one. The maximum aggregate size of these three spots, on the Michigan basis, would be $3 \times 4 = 12$.

All the beetles of Table I have been rated on this scheme. Of the Michigan beetles, no spotless individual had an aggregate rating of more than 2 for the three posterior spots, and no spotted beetle a rating of less than 6 for these spots. Of the California beetles, including those extracted from crosses, no spotless beetle rated higher than 3, no spotted beetle lower than 10, for the three posterior spots. When the classification of a wild population involves only the contrast between these two genes, there appears therefore to be no room for uncertainty.

Modifying Genes

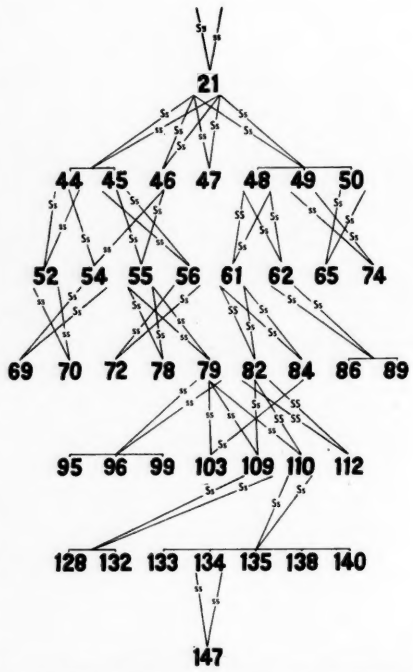
In certain lines of descent, however, the distinction between spotless and spotted is not so clear. One such line has been investigated through seven generations. In the light of data to be presented, the fluctuations observed in the pattern are interpreted as the result of a number of modifying genes.

The line referred to started with a female which when captured was regarded as spotless but which proved to possess the first four spots (counting from the front), and a male which was regarded as typically spotted though probably not having the maximum size of

all six spots. Both of these adults were lost, so that descriptions now are based on records and memory. The female may have mated before, but almost certainly with a spotted male if she had.

The course of the experiments dealing with the descendants is outlined in Figure 6. In explanation of that diagram, it is important to point out that the crosses made are not always the most desirable ones, but are the ones which succeed. In the long run, many attempted crosses fail. Though most early matings, starting with beetles taken in nature, are fertile and produce numerous offspring which mature, there is a progressive deterioration in later generations. Fewer and fewer matings are successful, more and more eggs fail to hatch, and mortality becomes higher and higher in all stages from early larvae to pupae. This decline in "vigor" has been noted in every series of experiments which I have so far conducted with coccinellids. The gaps in the series of experiment numbers in Figure 6 represent in general the experiments which failed. The reason may be inbreeding in the presence of semilethals, or the cumulative effect of unsuitable conditions in the laboratory.

Because of this deterioration, one is discouraged from waiting to establish homozygous strains before making crosses. Likewise, the separation, by selection, of various types out of an original complex genotype is largely prevented.



A GENEALOGY OF THE EXPERIMENTS

Figure 6

This chart shows the course of the experiments presented in Table II. The genotypes of some of the parents are only tentative. The gaps in the experiment numbers, in general, show the experiments which failed.

Detailed Data

For an understanding of the modifying genes involved in the experiments in Figure 6, it seems necessary to record the pattern of every parent and all of their offspring. This is done in Table II for the whole series of experiments. Each beetle is represented by six digits measuring its six spots on one elytron, beginning with the anterior outer (humeral) spot and zigzagging back, as indicated earlier, to the most posterior (apical) spot. The measure is on the basis of 4 for the maximum size yet observed when the scheme was devised, 0 for no spot, as explained in an earlier section. A strictly spotless beetle is thus rated 000000, a spotted one with maximum spots 444444.

Each enclosed space in Table II represents one experiment. At the top are the ratings of the two parents, the female being given first. Below, in two columns, are their offspring. The source of the parents may be ascertained from Figure 6. A question mark following the rating of a parent indicates that this beetle is no longer available, and that its rating is taken from recorded descriptions which in the early experiments were less accurate. In two early experiments a lost parent was recorded simply as typically spotted, and no rating is now possible. A number following and sepa-

rated by a hyphen from the rating of any of the offspring means the number of offspring having that particular pattern. In experiment 45, intended at first as a stock, the surviving parents are all males; the females were variously spotted. All other experiments started with a single pair, of which the female (except perhaps in Exp. 21) was virgin. The offspring are arranged roughly in order of aggregate size of spots, based primarily on the last three spots, secondarily on the first three.

Interpretation of Ratings

It is clear from such experiments as 110 and 133 (Table II) that the gene for spotless was borne by one of the parents in the first experiment (21), and that parent must have been the mother. From Experiments 70, 79, 95, 96, 99 and 147 it is apparent that the typical spotted pattern was also involved in that first cross. It is presumably a fair assumption that the array of patterns produced in most of the other experiments is the result of other genes modifying the expression of the spotless, or the spotted, or both. Which one is modified, and to what extent?

In the six all-spotted progenies named just above, the lowest aggregate rating for the three posterior spots, which as explained before are the most distinctive ones, is 10 (out of a possible 12). The lowest aggregate rating for the three anterior spots in these same progenies is 5 (out of a possible 12). If the posterior spots be accepted as the best criterion of the distinction between spotless and spotted, it may be argued that there is little evidence that the spots of the spotted pattern are reduced by modifying genes, and that any beetle whose three posterior spots had an aggregate size of less than 10 should be regarded as spotless. This argument would ignore the possibility that these particular six spotted progenies merely happened to be nearly free of modifying genes. The safest assumption appears to be, however, that most of the effect of the modifying genes is exerted on the spotless gene.

An approach from the opposite direction may be made by noting the ratings of individuals which must be genotypically spotless. This can be most safely done with the parents. As a convenience in later discussions, the supposed genotypes of all parents are given in Figure 6. These genotypes are judged from the patterns of the parents, their ancestry, and their progeny. Many of the genotypes shown are certainly correct; some depend on ratios of the progeny, whether 1:0 or 3:1, and are doubtful; and a few could just as well be something else—as, for example, the parents in Experiment 112 which might about as well be *Ss* as *SS*. Some of the most certain of the genotypes will be used to judge the extent of modification of pattern.

The mother in Experiment 44 must be spotless. So also must be both parents of 48, 50, 55, 61, 62, 65, 78, 82, 84, 86, 89, 112, 133, 134, 135, 138 and 140; one or both parents of 46, 49, 56, 69, 72, 128 and 132; the fathers of 47, 74, 103 and 110; and the mothers of 52, 54 and 109. The highest aggregate size of the three posterior spots which it is necessary to assign to any spotless beetle on the basis of the above judgments is 7 (father of 56). Five others have ratings of 6, though one of these is based on descriptive records instead of an available specimen.

The highest rating of a certainly spotless beetle is thus 7, the lowest for a certainly spotted beetle is 10, for the last three spots. Since the progenies include a number of beetles between these values, it is likely that the two patterns overlap, the darkest spotless beetles being darker than the lightest spotted ones. It would be impossible, if this is true, to separate the bearers of the spotless gene in all instances from the bearers of the spotted gene. However, because the purpose of these studies is to permit a determination of the frequency of the genes from a study of the phenotypes, it would be satisfactory to establish a point at which one variety is statistically separated from the other, such that judgments of beetles near this dividing point would be as often wrong in one direction

as in the other. Let us see what basis there may be for approximating such a point.

Experiments 21, 44, 52, 54, 103 and 109 assuredly involve the mating *Ss* × *ss* or its reciprocal. Their progeny should therefore be half spotless, half spotted, if the two types are equally viable. Of these six experiments, 103 produced too few offspring to be useful, by itself, in determining what rating of the spots would divide its progeny into two equally numerous groups. In the other five the median ratings are 6, 9, 8.5, 7 and 6, respectively. If all six of the experiments be combined, the median rating of their three posterior spots is 7.2.

An independent determination of the dividing point between spotless and spotted may be made from Experiments 48, 50, 55, 62, 65, 78, 89, 133, 134, 135, 138 and 140. In all of these families it is fairly certain that both parents were *Ss*, and the offspring should therefore be spotless and spotted in the ratio of 3:1. If these twelve experiments be combined, it is necessary to set the dividing point between spotless and spotted at 8.2 to arrive at a 3:1 ratio.

Any compromise between the 7.2 computed from *Ss* × *ss* matings and the 8.2 from *Ss* × *Ss* matings should take into account the fact that the former value is based on 123 individuals, the latter on 266. The standard errors of the 1:1 and 3:1 ratios based on these numbers are about 7.1 and 5.5, respectively, expressed in numbers of individuals. Without further statistical refinement, which would hardly be justified, I propose to adopt the value of 7.75 as the dividing line. In practice this would require that one-fourth of the beetles rated 8 for the three posterior spots be regarded as spotless, the remaining three-fourths as spotted.

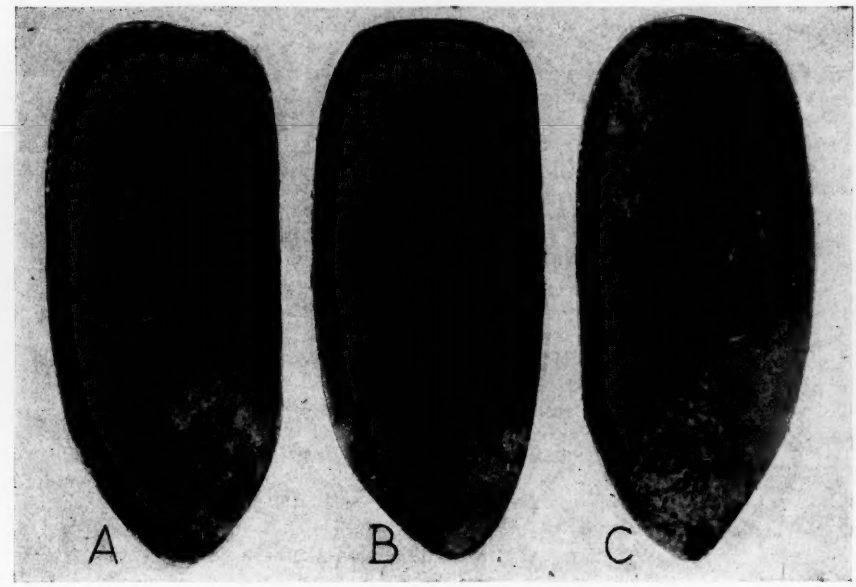
This criterion of the *S* and *s* genes may or may not be applicable to another line of descent whose modifying genes are different. In any case, it is applicable only when there is a considerable range of variation in a finely graded series. It is not applicable to a family or population in which there is a marked gap between a lightly spotted and a heavily spotted group, where perhaps some members of the heavily spotted group fall below the rating 8.

The Number of Modifying Genes

Any modifying genes which were possessed by the original mother of Experiment 21, and the male or males with which she mated, may have been very unequally distributed through their seven generations of descendants. It is of some importance to know the number of such genes contained in these beetles. To determine this point a standard of comparison is needed. What posterior spots could a spotless beetle have if it had no modifying genes? Experiment 82 indicates that probably it would have none, for in that experiment no individual, parent or offspring, has any of these spots. This conclusion is supported by Experiment 48, in which one parent is free of posterior spots, while the other parent has one very small one. The progeny of these parents are divided sharply into a spotless and a spotted class, with the darkest spotless beetle having each of the three posterior spots present faintly.

If complete absence of the last three spots be taken as the criterion of the absence of modifying genes, the progeny of Experiment 21 included two spotless individuals with no modifiers. The total number of spotless in this family is 21 or 22 if the above-described statistical criterion is accepted, or 16 or 17 if the family be equally divided. The spotted beetles presumably have their share of modifiers which cannot be detected in them. Thus, one-eighth to one-eleventh of the beetles have no modifiers. Assuming that the modifiers are in different pairs of chromosomes, so that they are distributed independently to the germ cells—which may not be true—one must conclude that there were three, possibly four, pairs of modifiers in the parents of Experiment 21.

No later generation has a very much smaller proportion of strictly spotless beetles (those with no posterior spots),



SPOTTED "SPOTLESS" WING COVERS

Figure 7

The dominance of the spotless gene is modified by other genes which permit some of the spots to "show through." In *A* and *B*, spot number 4 is accentuated; in *C*, spot number 5. In different parts of the range of this species different modifying genes appear to have become relatively fixed, altering the accentuation of various spots in different regions.

hence none need be supposed to have more than four pairs of modifiers. It would be easy to conceive of dominance relations which would permit an individual of a later generation to have more induced spotting than any of the progeny in Experiment 21 would be likely to have; but such an arrangement is unnecessary. Experiment 55 includes about the same proportion of unmodified spotless beetles as does Experiment 21, but most other experiments which have spotless individuals at all have more than one-sixteenth of them without posterior spots. These latter progenies must be supposed to have fewer of the modifying genes. Experiment 82 may have lost all of them, 110 and 133 all or most of them.

Qualitative Differences Among Modifiers

Whether the genes that induce spotting in beetles with gene *S* are equal in their effects or whether each one affects particular spots more than others, is a question not necessarily raised in the mere determination of the number of such genes. There are some indications that the modifying genes may affect certain spots. The spotless progeny of Experiment 45 shows a strong emphasis on spot No. 4. One of the progeny of this experiment, the mother of Experiment 55 (which herself shows this emphasis on the 4-spot), together with a male from 46, which shows a smaller emphasis on spot No. 4, transmits a still observable prominence of the same spot. Another line of descent from 45 (Exps. 56 and 72), however, shows practically no accentuation of spot 4. The progeny of Experiment 52 show a similar emphasis on spot 4, derived possibly from their spotted father which would not show it. Others show a less marked in-

fluence of the same sort. See Figure 7, *A* and *B*.

The same spot (4) is, however, influenced less than the other posterior spots in some experiments, as in 61 and 89, where spot No. 5 receives the emphasis (Figure 7*C*). In none of the experiments is the sixth spot singled out for increase by the modifying genes.

Frequency of Genes in Populations

On the basis of the judgments arrived at in the foregoing experiments, several populations have been analyzed to determine from the phenotypes the frequency of gene *S* (spotless) and its modifiers. Three such populations, furnishing the original parents in the experiments recorded in Tables I and II, have been mentioned. Others have been obtained from Gold Hill, Colorado (from Prof. M. A. Palmer) and Jumbo Mountain, Colorado (from Prof. Gordon Alexander)—both of these localities relatively near Boulder; and from Sonora, California (Mr. C. E. Gustafson). I am greatly indebted to the persons named for their assistance. The largest collection from Michigan was obtained at Coloma, Michigan, through the courtesy of Mr. Tom Daggy. Numerous smaller collections come from all over the Lower Peninsula of Michigan.

The beetles in each collection have been classified as spotless and spotted. Some collections of hibernating beetles were so large that only a fraction was studied; it seemed unnecessary to classify more than about two thousand beetles from one area. The numbers of spotless and spotted from each area are recorded in the first four columns of Table III.

From these numbers has been computed the frequency (p) of the gene *S*, using the formula $p^2SS + 2p(1-p)Ss + (1-p)^2ss$ as representing the frequencies of the three genotypes. This formula rests on the assumption that those genotypes lead to equal fertility, viability and longevity, and that matings involving them are made at random. The percentage of the pertinent chromosomes which contain the gene *S*, which would also be the frequency of the gametes containing that gene, is recorded in the fifth column of Table III. Gene *S* is somewhat more abundant in Colorado than in Michigan, but conspicuously less frequent in California.

The three localities in California have sufficiently different frequencies, and their geographic position is such, that a gradient is indicated. It will be of interest to obtain populations of other areas near them to discover what becomes of this gradient.

Frequency of Modifiers

Of no less interest than the distribution of the spotless gene is the frequency of its modifiers. It is clear from the relative numbers of spotless beetles having posterior spots that the several populations in Table III are not alike. A single numerical value representing the frequency of the modifying genes in each population would be desirable, but it cannot be ascertained until the genetics of these modifiers is known in some detail. Almost any set of assumptions regarding their inheritance would show the several populations to be unlike, though the nature of their differences might be incorrectly indicated. Perhaps it is worth while to use one set of assumptions.

Let it be assumed that there are modifiers of spotless at four loci (the largest number indicated by the experiments with Michigan beetles); that each modifying gene is at least partially dominant, so that a beetle heterozygous for even one of them would show some posterior spotting; and that the four modifying genes are equally abundant (which is probably incorrect). It is not necessary to stipulate that these loci be independently distributed, since in a long-time process like evolution the recombination of genes is not restricted by linkage.

To make computations from these assumptions it is easiest first to ascertain the proportion of the population which carries *no* modifying genes. This can be done only with the beetles which carry the gene *S*, since the modifiers, so far as known, have no effect on the spotted pattern. The eighth root of this propor-

tion will be the frequency of the gametes which do not carry a particular modifying gene—any one of the four, since we are postulating equal frequencies. Subtracting this root from one gives the frequency of each modifying gene.

On the basis of these assumptions, 15.73 per cent of the pertinent chromosomes of the beetles from the Lower Peninsula of Michigan should carry the first of the four genes that induce or permit posterior spots in a beetle having gene *S* (see last column of Table III). A similar number of pertinent chromosomes would contain the second of the four modifiers, and so on. Gametes, or even chromosomes, might include two or more of the modifiers.

The populations from Michigan and Colorado do not, on these assumptions, differ greatly in the number of modifiers of spotless; but the number in California is strikingly lower. Almost any set of assumptions regarding the modifying genes could hardly fail to show some kind of difference between the populations of Michigan and California.

Discussion

It is to be noted that no use of the three anterior spots of the pattern has been made in these studies. There are indications that differences in these have a genetic basis, and that variations in them may be as distinctive of geographic groups as are those of the posterior ones. They have been omitted from a first study because they are not as easy to use in an analysis of populations.

The single set of assumptions regarding modifiers of the spotless gene should not be taken as an indication that all populations are regarded as falling into the same scheme. Observation makes it highly probable that the Colorado population has other modifiers than those in Michigan. Certain combinations of emphasized spots which are frequently repeated in the former area are uncommon or absent in the latter. This must mean a difference in the modifying genes.

Attention should also be called to the fact that the rule for recognizing the spotless beetles in Michigan was in one respect violated in its application to Colorado phenotypes. According to that rule one-fourth of the beetles which had an aggregate rating of 8 for the three posterior spots were to be regarded as spotless, the remainder as spotted. In the Colorado population the rating 8 was not infrequently attained by having two of the spots fully developed, the third one wholly lacking. The two full-sized spots might be either 4 and 5, or 5 and 6. The Michigan beetles seldom showed this type of pattern; the spotless beetles seldom had even one posterior spot fully developed, two of them practically never. It was feared that this peculiarity of the Colorado beetles is a modification of the spotted pattern, not of the spotless. Accordingly all beetles which got their rating of 8 from two large spots, with the third missing, were classified as spotted. There was no opportunity to ascertain the genetics of this pattern after it was observed.

There is little literature concerning the pattern of *Hippodamia convergens* in relation to distribution. This paucity has been mentioned in an earlier article.[2] Dobzhansky[1] states that omission of some of its spots is less common in the eastern part of the United States than in California. The studies here reported show that this difference is not the result of a lower frequency of the spotless gene in the East, for actually it is more than twice as common there as in the far West. It is caused by the modifying genes which restore to the eastern beetles some of the spots of which the spotless gene alone would deprive them.

Summary

The spotless pattern of *Hippodamia convergens* differs, in its simplest manifestation, from the spotted pattern by just one gene. The spotless gene is almost completely dominant. It may be accompanied by modifying genes which restore some, or even all, of the spots, though in reduced size. The two patterns are most sharply distinguished by the three posterior spots, but there is some overlapping even of these. In the presence of modifying genes the largest

TABLE II

Experiments with a line of descent involving modifiers of Spotless

Exp. 21

1111007 X	spotted
001000	122222
101000	212222
000010	121222
101110	122231
022110	101133
023110	222322
333110	444232
222020	111323
122211-2	123233
023022	332333-3
111221	344144
002122	222343
022222	444444-5

Exp. 44

123222 X	spotted
111000	222044
023110	1113333-3
101011	3333333-2
001200	343244
101320	3333344-3
202222	2224444-2
222222	4224444-7
222033-4	3344444
111322	4444444-3
2221133-2	

Exp. 45

	023022
Spotted	022222
in some X	111323
degree	344144
	222343
000000-2	122212
000100	111411
101300	111133
111300-2	112322
022300	022122
000220	022333-4
011310	222333
201310	303333
220400	1114444
211202	4224444-5

Exp. 46

122231 X	101133
100000-4	222220
111100	421220
001200	222320-2
011210	222233
202300	222431
100121	111333
101310	322333-2
202031	343244
220220	4224444-4

Exp. 47

123233 X	023110
011000	001333
023000	222333
011010	034333
032010	4444444
023021	344454
123121	443454
134231	444454

Exp. 48

001000 X	000010
000000-5	333333
001000-12	412443
010000	412444
001010	422444
201010	434454
101111	

Exp. 49

122222 X	002122
010000	111233
001010	123333
012010	244343
033110	2333344
012111	1134444
000222	322454
021222	

Exp. 50

022110 X	101110
001000	133221
011000-4	211231
011100	032231
121100	033232
012010	220430
211010	033332
212110	403333
011200	322444
001210	434344
111210-2	4443444
121210	2324444
111120	4214444
202120	4224444
033120	4234444-2
122121	4414444
233220	3344444
000131	2444444
122221	4444444-2

Exp. 52

023110 X	334444
011300	101233
011311	222333
021312	012433
122321	302444
023321	2224444
133321	2334444
211322	4334444
123322	4444444-1
213322	

Exp. 54

001200 X	343244
001100	101331
201210	212232
213210	323223
011112	102332
011222	2124422
113321	134233
223321	123333
011322	234434

Exp. 55

201310 X	011210
000000-2	200221
011100	011212
111100	201320
111110	321233
100200	100431
010210	111434
110210	322344
111210	332443
001310	321444
101310	2224444-3
111310	4134444
200211	4124444-2
011122	1234444
111122	4224444-2
111311	

Exp. 56

1113237 X	022322
011100	011233
010200-2	000234
010111	222333
011321	010433
010223	0004434
011223	2214444-2
010232	4114444
011322	4214444
010421	3444444

Exp. 61

000000 X	001000
000000-24	010021
100000	212121
001000-3	311121
010000	211131
101000	312131
001010	2111221
101010	1111132

Exp. 62

3010307 X	201010
000000-2	001220
001000	101220
101000-2	201131
011000	112131
101010-2	212131
202010	301142
100100	222233
011100	313343
200100	4123444
102110	2124444
301110	2134444
201020	4124444
101120	4234444
112120	4344444
111121	2124534

Exp. 65

111210 X	212110
100000	312220
111000-3	133220
212000	211321
4111110	4123430
000111	3111331
211111	2224430
212111	3113333
012210	1334234
210210	4114343
023300	4013444
211211	4223444
211220	4344444-2

Exp. 69

213210 X	223321
111000	223310
022100	223221
122110	1343214
212110	3444214
111200	2323334
033200	4444444

Exp. 70

4444444 X	434444
344244	3334444-2
4233444	4154444
4343444	3444444-3
4644344	4344444
4224444	4444444-5

Exp. 72

222333 X	0102227
010000	122334
122134	1103444
111334	4214444
011334	4314444

Exp. 74

113444 X	000222
112111	212232

Exp. 75

001310 X	110210
000210	211332
022300	222423
011410	3443344
213032	3104444
122322	4344444

Exp. 79

3343447 X	4334347
4103444	4224444
4213444	4234444-2
3333344	4334444-3
4333444	4344444
4104444-2	4434444-2
4214444-3	4444444-11
4314444	

Exp. 82

000000 X	101000
000000-26	001000-2
100000	110000
010000-5	011000-2

Exp. 84

1010007 X	100000
100000	

Exp. 86

202020? X	202020?
111010	

Exp. 89

202220? X	2010207
201030	202131
312030	4233343
202041	

Exp. 95

4314447 X	4344444
4104444	4234444
4114444	4324444
4314444	4444444-2
4224444	

Exp. 96

4444444 X	4324447
4024444	4334444
4314444	4444444-3

Exp. 99

4444444 X	4334444
4444444	

Exp. 103

4444444 X	100000
022020	

Exp. 109

1100007 X	4444447
000000	244232
022000	133143
033000	2002443
000010-2	2113333
012010	3103333
223221	4223333
344230	2233443
122133	

Exp. 110

4434444 X	000000
000000-14	021000
010000-4	001010
011000-5	011010
110000	033010

Exp. 112

110000 X	100000
000000	

Exp. 128

344230 X	033000
000000-2	232221
010000-2	132143
011000-3	4112444
020000	4213444
021000-2	3343344-2
032000	4443444-2
122021	4444444
243220	

Exp. 132

022000 X	122133
020223	

Exp. 133

000000 X	011000
000000-6	011000
001000-3	020011
010000-5	

Exp. 134

000000 X	001010
000000-20	2333344
000010	4323444
010020-2	4314444
2213444	

Exp. 135

1020107 X	1000007
000000-10	011010
010000-4	4213444
001000-2	3333444
011000-7	4433344
1001010	4444444

Exp. 138

000000 X	000000
000000-11	4213444
010000	2324444
3422444	4324444

Exp. 140

1110007 X	000000
000000-2	4434444
2.0000	

Exp. 147

4314444 X	2213444
2222444	3224444
2213444-2	4214444-3
3213444-2	4314444
2223444	3324444-2
3313444-2	3414444-2
4213444-7	4224444
4313444-2	4324444-4
4423444	3434444-2
2414444	4424444-2
3313444-3	4344444

posterior spots of a "spotless" beetle may be larger than the smallest posterior spots of a spotted beetle. For statistical purposes, the two patterns may be separated in instances of overlapping at the point where the posterior spots have about 65 per cent of their maximum size.

Analysis of natural populations indicates that the spotless gene is present in 6 or 7 per cent of the pertinent chromosomes in Colorado and Michigan, but only in 2 to 3.5 per cent in California.

The modifying genes appear to have no effect on the spotted pattern. From the frequency of spotless beetles which have no posterior spots, and which are assumed to have no modifiers, it is computed that three or four pairs of modifying genes have been involved in one experiment. Some of the descendants of these experimental beetles certainly had fewer than four pairs, some probably none at all. The modifying genes appear to be unequal, one of them relating more especially to spot No. 4, another to No. 5.

On the basis of certain assumptions an estimate of the frequency of the modifying genes has been made. The estimates indicate that the modifiers are nearly three times as frequent in Colorado and Michigan as in California.

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TABLE I

Experiments showing inheritance of spotless and spotted patterns in Hippodamia convergens

Origin	Exp. No.	Ancestry of Parents ♀	Ancestry of Parents ♂	Nature of Parents ♀	Nature of Parents ♂	Offspring Spotless	Offspring Spotted
	20	Wild	Wild	Spotless	Spotted	21	12
Ann Arbor	37	F1 of 20	F1 of 20	Spotless	Spotless	12	3
	38	F1 of 20	F1 of 20	Spotless	Spotless	31	5
	39	F1 of 20	F1 of 20	Spotless	Spotless	51	19
	36	F1 of 20	F1 of 20	Spotted	Spotted	0	73
	52	F1 of 20	F1 of 20	Spotted	Spotted	0	10
	53	F1 of 36	F1 of 36	Spotted	Spotted	0	24
Placerville	264	Wild	Wild	Spotless	Spotless	24	9
	265	Wild	Wild	Spotted	Spotted	0	15
	266	F1 of 264	F1 of 265	Spotless	Spotted	19	0
	268	F1 of 264	F1 of 265	Spotless	Spotted	16	0
	270	F1 of 264	F1 of 265	Spotless	Spotted	17	0
	267	F1 of 265	F1 of 264	Spotted	Spotless	9	0
	269	F1 of 265	F1 of 264	Spotted	Spotless	15	0
	271	F1 of 265	F1 of 264	Spotted	Spotless	14	0
	272	F1 of 265	F1 of 264	Spotted	Spotless	13	0
	273	F1 of 265	F1 of 264	Spotted	Spotless	11	6
Extracted	307	F1 of 295	Wild	Spotted	Spotless	6	5
	319	F1 of 307	Wild	Spotless	Spotted	18	13
	320	F1 of 307	Wild	Spotless	Spotted	12	9
	324	F1 of 320	Wild	Spotless	Spotted	17	21
	325	F1 of 319	Wild	Spotless	Spotted	11	12
	322	F1 of 319	Wild	Spotted	Spotted	0	20
	323	F1 of 319	Wild	Spotted	Spotted	0	40
	326	F1 of 320	Wild	Spotted	Spotted	0	16
	327	F1 of 320	Wild	Spotted	Spotted	0	29

TABLE III

Frequency of the spotless and spotted patterns, of the gene for spotless, and of the modifiers of spotless, in Hippodamia convergens in several localities

Locality	No. of Spotless Beetles: Without posterior spots	No. of Spotless Beetles: With posterior spots	No. of Spotted Beetles	Percentage of Pertinent Chromosomes with Gene S	Percentage Frequency of Four Modifying Genes
Lower Peninsula, Mich.	87	295	2674	5.84	15.73
Coloma, Mich.	61	197	1808	6.45	12.49
Gold Hill, Colorado	87	237	2141	7.02	15.16
Jumbo Mountain, Colo.	36	95	831	7.06	14.91
Yosemite Valley, Calif.	91	69	2118	3.58	6.81
Sonora, Calif.	79	32	1846	2.86	4.16
Placerville, Calif.	64	23	2063	2.04	3.77

Population Pressure in Formosa

It is obvious that no population inhabiting a limited and already densely settled area, and dependent on agriculture, can reproduce indefinitely at the rate indicated by a gross reproduction rate of about 3 and a net reproduction rate of about 2. The potential growth of the Islanders of Formosa is even more rapid than is indicated by these rates, since any social or economic changes that would promise an eventual lessening of fertility would produce immediate and more rapid declines in mortality. The actual growth may be much less rapid than the gross and net rates indicate, since the collapse of order and the discontinuance of the sanitary measures imposed by the ruling Japanese caste may eliminate the minimum control of mortality achieved during the Japanese period. The war will remove Formosa from the Japanese Empire, and thus alter the particular nexus of causal factors responsible for the extraordinary growth that occurred between 1905 and 1940. The problems of the economic and demographic future of Formosa will be minor and dependent aspects of the greatest of all Asiatic problems, the future development of the economy and the population of mainland China.—*Population Index*, July, 1944.

MILK, HONEY, AND LIBERTY

Or, the Sacred Purebred Cow Jumped over the Bureaucratic Moon

A Review

THE Peck's bad boy of the dairy cattle breeding industry, Mr. Parmalee Prentice of Mount Hope Farm, has added a new book to his previous string of six. Two were concerned with the law, two with hunger, and two with the sometimes funny and frequently alarming gap between genetic insights and livestock breeding practices. His latest book* is thus appropriately divided into three parts like that Gaul which our author depicts as a region where neither Caesar nor Charlamagne found any spotted Holsteins worth mentioning. It deals with the three subjects covered in the earlier books, hunger, cattle, and the law, as they affect the shrinking and dynamic world of 1944, and as their interweaving in 1944 determine the pattern of the future. His synthesis of these tenuously related matters into a bio-legal "wave of the future" is argued in the best tradition of the bar, but eloquence is not enough to overcome certain glaring semantic difficulties.

Even though Homo has been a relatively scarce species through most of his days on earth, Prentice has frequently reminded us that hunger has been endemic in the human race since the first distant beginnings. Humans have been adequately fed only briefly in small areas, and even then only a few over-privileged classes have been lucky enough to eat regularly. In all ancient civilizations, perennial semi-starvation is pictured as the fate of most of the people most of the time. This was true all through the Middle Ages in Europe. Only with the advent of new techniques in agriculture and of an Industrial Revolution has mankind achieved in the Western World what Prentice calls a "century of grace." For three or four consecutive generations whole populations actually have not been hungry. Prentice says that such a dietary breathing spell has never happened before. It has released tremendous new energy of mind and body—for malnutrition does not make geniuses. Our author quotes a pathetic passage in a letter from a gifted Frenchman who has been fated to watch his own mind deteriorate under the "New Order." "I am humiliated to confess that we never speak of anything but food. We think of food all the time. We are able to get enough to keep us alive, but not enough to work as we should, and since we must work, I am conscious that my mind is confused and that what I write is confused."

Prentice is convinced that only a miracle of quick thinking and ingenuity will long delay the ending of that "century of grace." As matters now stand, a new night of hunger will inevitably return, and the humiliation of struggling against hunger-warped minds will be universal. This pessimistic call for a planned miracle is documented by a résumé of statements by many distinguished students of population and food supply. The author sees no hope that a miracle of chemurgy will set things right. The arable land of the world comprises just so many acres. On some five million square miles of arable dry and not-too-dry land in the world, a tenth actually is good wheat land, etc. Farmed in various ways, certain millions of bushels of grain will be harvested, certain millions of livestock will be ready for slaughter each twelvemonth. Improve agronomic techniques and develop new crops and the total is upped, by a quarter or a third, or even a half. But that does not keep up with increasing numbers of human beings over the past century. It does not keep up with the present increase of several tens of thousands per day the world over. Hybrid corn in 1943 is credited with increasing the Corn Belt yield over half a billion bush-

*Prentice, E. Parmalee: Food, War, and the Future, pp. 164, $2.50, Harper & Bros. 1944.

els—a quarter of the U. S. crop. That helps, surely, but it is not good enough to allow us to dismiss the question of numbers of people as something that will take care of itself. In Eastern Asia, where half the human race lives, half a billion bushels of corn (or an equivalent of food-calories acceptable to the Oriental palate) would vanish like August snow in Washington. To ignore numbers, Prentice holds, will insure a quicker return of that ancient hunger and the mental prostration culminating in the inevitable doctrine of despair and submission — the "apathia" of those whose starving bodies starve the mind and make an end of hope.

On the question of population control, Prentice has little to say beyond "or else." And Puerto Rico still stands as a front-yard demonstration that our administrators do not have an answer, or dare not use it. It seems to be much easier to hope for a miracle of loaves and fishes that it is to apply, in the precious time between starvings, the miracle of the human mind to evolve a real basis for peace and plenty for all mankind.

Sykes Goes Bail for Grimes

The second chapter deals with the agricultural palliation of this alarming situation, through increased production, and the evolution of a new agriculture for the 20th century. The answer, readers of Prentice's earlier books might guess, is a drastic housecleaning of the livestock breed associations and a revolution in their aims, methods, and public relations. In his earlier books, in advertisements in breed journals, and in articles, he has done a thorough and documented job in proving the absurdity, from any rational genetic point of view, of much pure-bred hocus-pocus. In this book he presents an able summing up of his position. The so-called "pure breeds" of livestock represent *breeding isolates* relatively homozygous mainly for certain color and conformation genes. The breeds are not actually "pure" except in a merely formal sense that their ancestors came to America on certain boats. The entire pure-bred concept is based far too much on a transfer of a D.A.R. psychology to livestock, and far too little on the genetic realities.

Prentice does a thorough and documented job of proving that the claim that the cattle breeders of the Low Countries and of the Channel Islands have maintained a program of pure breeding for centuries is simply hocum. The existence of an army of "purebred scrubs" he considers an indictment of the honesty and acumen of the purebred industry. He contends that the animal husbandmen of the State Colleges and Experiment Stations, and the U. S. Department of Agriculture are little better than intellectual prostitutes:

> Henry Fielding thought it much easier to make good men wise than to make bad men good. The history of American agricultural colleges raises some doubt as to this conclusion. It has been impossible to make good men in the colleges wise. For sixty years these colleges have seemed to be under the dominion of fanciful notions, alien to the spirit of scientific knowledge, and this alien dominion has influenced the speech of teachers and apparently controlled their very thoughts. The word "pure-bred" has been so often repeated that it has been believed, and when registered cattle sell for high prices—frequently for many thousands of dollars—the imagination of teachers in agricultural colleges is fired with enthusiasm. Their teaching, in part, is responsible for these high prices, and instead of putting themselves in the place of the purchaser who has been led to waste his money and in many cases to ruin his business, the colleges look at the sales as triumphs for their own ideas, as, indeed, they are.
>
> The Colleges, then, stand athwart the path of progress.
>
> It will be easy, by references to professorial writings, to answer the charges of ignorance and credulity made against many teachers and professors of past and present time. Sykes will go bail for Grimes, and Grimes will go bail for Sykes. If, however, the mutual benefit system be not available to the gentlemen now before the court, what can they do when they must meet historical questions by the methods open to historians?
>
> The urgency of the question is constantly increasing. In their departments of animal husbandry the colleges need, and before very long will have, modern biologists. Modern Bakewells are an anachronism and intolerable.

These are fighting words. If the "intellectual turpitude" (or turbitude?) is as serious as Prentice's charges, higher education in these United States must

be in a parlous state. For the dry rot of the animal husbandry departments must have seeped, at least a little, through departmental bulkheads. The maintenance of fictionally "pure" cows in college pastures might well be less serious than the maintenance of fictional "sacred cows" in other branches of knowledge. And neither the cows nor the "sacred cows" seem to be on the verge of passing away: "The maintenance of 'pure-bred' herds of dairy cattle by various agricultural colleges has always been wrong, and is now beginning to be absurd. When popular understanding of the actual situation has advanced a little farther, these herds will be discontinued. This is not likely to come soon, for the colleges seem to be unable to reform themselves and the public is not yet sufficiently acquainted with the facts to compel reform."

Prentice has said this before. If he is wrong, one wonders why those he has attacked do not make him eat his words in a libel suit. If he is right—and his arguments have never been answered—some drastic housecleaning is overdue.

Freedom—1776 and 1944

When Prentice undertakes to discuss human liberty and "democracy" in America in his concluding chapters he seems to fall into the same pit he has so ably dug for the livestock people. These chapters discuss the part played by the traditional "America way" in the evolution of human freedom. "Freedom is the greatest gift which the government can bring, and the rarest. It is the source of hope and ambition, of energy and initiative. In America it has brought the abundance for which our country has been remarkable, and it makes progress possible and life worth living."

These important goals can be attained, Prentice believes, only by a return to Jeffersonian simplicity — the best governed people being the least governed. "In America the problem [of freedom] was solved by recognition of the inalienable rights of men, giving these rights into the protection of equal laws." Universal yearnings for ease and for exercising arbitrary power are pictured as furnishing the drives whereby these rights are eternally in jeopardy from government encroachment. If the bureaucrats don't take your freedom with a gestapo squad, they filch it with a food subsidy ticket. "Hence all government activities which interfere with a *laissez faire* economy, or which sop initiative through social security, are equally a danger to the liberties the founding fathers prepared for us." Thus a Beveridge Plan would result in "the condition described by Mr. Gladstone's word ptochocracy and leads to a condition for which he might have used another extraordinary word, ptochogony — an increased birthrate among paupers, — and so more want and more fear until there is more relief and larger pensions. When the voters control their own property, they protect themselves from want and fear. Ptochocracy is not freedom of any kind, — it is nothing else than the old autocracy of despot and followers."

In his eloquent plea for a return to the halcyon days of *laissez faire,* Prentice almost brings tears to the eyes of his readers with his vivid picture of an octopus-like government sucking the life blood of courage and initiative from the once rugged American people. In the light of "the record" on Saipan, in Normandy, on the sea and in the air, this picture seems distinctly overdrawn. That those who went on relief during the depression promptly spent their time bulging the birth rate is not supported by the few careful studies that have been made. (See, for example, JACOBSON, P. H. *Milbank Mem. Fund Quart.* 23:131-144.) A distressing feature of early eugenic homilies was the tendency to view with self-righteous alarm the birthrate of the underprivileged, blandly ignoring the fact that these forgotten people were barred by law and custom from access to the knowledge which might enable them to control their fertility. Until our culture puts an end to this cruel hypocrisy, such statements as that above quoted are bad taste and worse logic.

Prentice holds that the American people have foolishly and supinely relin-

quished ancient wisdom for a mess of bogus security. No bedtime story could be cast in simpler terms than this picture of the ignorant and stupid departure of the American people from these primary virtues. So simple is it all that in a chapter of 25 pages bemoaning our fate, the author never once even hints that the disastrous 1930's had any other cause than a repudiation of the commandments of liberty promulgated in 1776. A man from Mars, catching up on world affairs through this book, would never guess that huge new aggregations of non-governmental powers emerged during the "century of grace" and that the battle for the preservation of the inalienable rights of men became an unequal three-cornered struggle between two giants and a rather pathetic pigmy, good John Citizen, armed with a ballot box and entrenched behind a cracker barrel. The individual citizen, the local governments and even the States have not been too successful in maintaining their fundamental liberties against the encroaching powers of the increasingly centralized Federal power and against the growing strength and ruthlessness of those other socio-political giants, the corporations. To try to picture the interlocking reactions of these vortices of political and economic force is outside the scope of this note. The point is, that the reader of Prentice's book would never discover that articles of incorporation had ever been granted to any group of Americans since the Boston Tea Party. This vast white space in the discussion cannot be because the author of *The Commerce Clause of the Federal Constitution,* and *Federal Power over Carriers and Corporations* never heard of the problem.*

Elsewhere Prentice says: "When the voters control their own property they protect themselves from want and fear." During the last half century, which of the giants has exercised the greatest control of people through controlling property? In those golden half forgotten days before the New Deal, J. J. Raskob's General Motors "made 90 millionaires," but in the process thousands of individual American citizens contributed willy nilly their "ownership of means of production" to the creation of as rigid an "autocracy of despot and followers" as any which Prentice deplores. The ownership by corporations of the means of production of millions of American workers over large areas of our country has at times abrogated the liberties guaranteed in 1776 as completely as control from the bureaus in Washington. To talk about "the American way" in a "company town" is obviously nonsense; even Rockefeller in-laws must have heard of Leadville. During the preparations for the worldwide battles we are waging today, there have been times when even the power of the Federal Government

*Since writing the above, the reviewer chanced upon a statement by Dr. Robert Lynd (*Amer. Sociol. Rev.* 9:15, February 1944), which from a different approach emphasizes the point that the concept of "separation of powers" has serious practical limitations. Dr. Lynd writes as follows: "I believe the United States along with the rest of industrial society, is now moving rather abruptly and whether we like it or not through an institutional revolution, and the old institutional system that has shaped our thinking as social scientists and the statement of our problems as scientists, is falling away from under our feet. I believe we are changing over from an institutional system in which desired things were assumed to happen *in*directly and as a by-product of working for something else, to a prevasively different system oriented around direct and centralized planning to make desired things happen. The old 'American way,' now on its way out, operated on the principle of 'Each for himself and God for all of us, as the elephant said as he danced among the chickens.' And this humanly wasteful way evoked prodigies of raw energy and enterprise, i.e., was efficient by its own primitive standards, in the loose-jointed frontier era of grab and exploit. It sought to provide employment and needed goods and services and to keep an increasingly productive technological apparatus at work as a by-product of the private scramble for property. It sought to provide a social system by allowing society to heap itself up casually, anyway, in the wake of this atomized competitive striving for money. It sought to operate a democratic state by the fiction that power is divisible, that liberal state power can stand apart from, and yet whenever necessary dominate, other systems of power such as that implicit in economic institutions."

has been strained to control some of these corporate giants. The obsession of the aluminum and steel industries with plant capacity nearly lost the war for us. And yet nothing of this vastly important sector of our national life is even hinted at in Prentice's discussion of the forces which shape our future.

To talk of liberty in such a curiously one-sided vacuum is fully on a par with repeating the ancient myths about Jersey cattle being pure-bred in a watchful isolation for a thousand years—and a great deal more dangerous. One who has spent a lifetime studying corporation law and the impact of constitutional limitations on corporation activities, seems thus to be in a peculiarly vulnerable position when he combines these views with an attack on the "pure-bred" ballyhoo of the breed associations and the experimental stations.

In noting these objections the reviewer must say that he is hardly less apprehensive than Prentice about the danger of destruction of our liberties by government fiat. He differs in seeing an even greater danger in the less direct, and hence even more sinister, control by large non-governmental aggregations of power. The greatest danger of all is obviously in alliances between these two giants.

In objecting to Prentice's argument the reviewer is not "going bail for husbandman Grimes." He merely wrings his hands as he notes in great perplexity that neither Grimes the animal husbandman, nor Sykes the lawyer has deliberately set out to mislead anybody. In their different ways, both of them actually believe the particular array of quaint folklore they have grown up with, and adopted as the ideational furnishings of their minds. The fable of the thousand years of pure bred cattle in the bogs and islands on the edge of Western Europe is absurd, but it is a relatively harmless sociological myth. It may degenerate into a minor racket to mulct tired businessman bent on farming, or, more seriously, retired men and women whose savings sometimes disappear in a herd of "pure bred scrubs." But this shoddy business hardly endangers the preservation and extension of human liberty. The curious mixture of incompatible ideational bedfellows displayed in this book is an excellent example of that unfortunate gullibility of human beings which so often leads them to believe, to their proximate or ultimate hurt, what they want to believe. The particular "sacred cows" we adopt as members of our private Pantheons are immune from self-criticism by those who harbor them. Prentice's complete sincerity is demonstrated by the fact he is willing to back this curiously one-sided argument with his name and reputation.

Prentice seems to have the pure bred cattle industry on the hip. Unfortunately to clean the Augean stable of the show-ring and the herd books would not solve the food problem. He is right that our centers of learning must do a better job. It is vastly more important that they do a better job in establishing a rational understanding of the bases of our liberties than that they make cattle breeding scientific. To the reviewer, such disconcerting semantic blind spots are alarming—and they are distressingly frequent. It is this affliction that Korzybski has pictured as the major problem of our time. Some feel that in his enthusiasm for "non-aristotelianism" and the principle of non-identity. Korzybski sometimes himself falls into the pit he is helping humanity escape from. Be that as it may, if through non-identity or by the use of an "anthropometer" or in any other way, the patient (the human race) can be cured of his deplorable tendencies to fact-blindness, such a catharsis is urgently to be welcomed. Could this be done, the miracle of the "good" becoming wise and the "bad" becoming good, might in a trice be achieved. Surely this book is an eloquent demonstration of the need for such a miracle. It is most unfortunate, however, that by mixing politics and livestock genetics Prentice has detracted from the very important contribution he made in emphasizing the need for facing genetic realities in livestock improvement—ROBERT COOK.

AGE AND HAIR-FORM

Change in Size and Shape of Maya Head-Hair with Age

HENRI C. SEIBERT AND MORRIS STEGGERDA
Carnegie Institution of Washington, Cold Spring Harbor, N. Y.

IN previous studies on hair in this laboratory[4-7] it has repeatedly been emphasized that human hair varies greatly in size and shape. This variation has been associated with race, sex, and region on the shaft where the hair is cut. The present study has to do with the change that hair undergoes with advancing age, both in size and in cross sectional shape.

Oesterlen (quoted from Kneberg[2]) showed that in the same race, the diameter of hair varies with age of the individual from very fine in childhood to a maximum in the adult and then decreases in old age. Sex variations in the diameter of hair were insignificant in adults. Trotter[9] found that the diameter of facial hair of both sexes increased with age; the most marked increase in thickness of hairs on lips and cheeks was in passing from the 11-20 year group to the 21-30 year group. Trotter[10] further found that:

> [The] diameter of hairs (hair of legs of white and colored individuals) increased gradually with increase in age. The pubic and axillary hair showed a definite increase in length and average diameter only when passing from the under 20 year old group to the group between 20 and 30 years and remained relatively constant in the older groups. Minakow (cited by Martin[3]) found hair of females exceeded in size that of males in the same groups. According to Danforth[1] the hair at birth is fine and silky and during the first few years of childhood it becomes coarser, the average diameter approaching that of the adult. At a more advanced age, he continues, the hair again tends to become fine, sometimes assuming a silky character like that of an infant. The average diameter of the larger hairs he quotes as having been said "to be slightly greater in women than in men."

Kneberg[2] found that the hair of Syrian Druzes had the greatest mean weight in the age group 20-30 years. Since hair weight is closely associated with its area, it may be assumed that the hair was largest at that time. Trotter[11] showed that the index was highest in males in the 0-9 age group with a second peak at 60-69, but in females it was highest at 60-69 with the second highest peak at 20-29. From 20 years up the indices for males was lower than females except in the age period 70-79 where the numbers were too small to have made this a true situation. In area she found that the hair rapidly increased from the first ten years period to the second followed by general tapering off in size. When the 0-9 group was broken up into two groups of five years each, it was found that the youngest group (0-4) had extremely fine hair. No significant sex differences were noted except in the 0-4 group in which period the females had larger hair. In a series of French Canadian hair Trotter and Dawson[8] found that the area increased with age up to 20 years and decreased after 50.

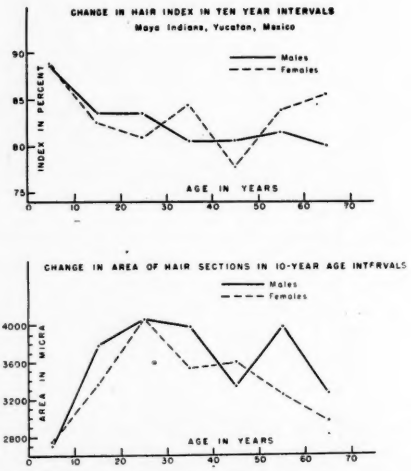
Wynkoop[12] measured 483 hairs of 82 persons from three hours to 91 years of age and concluded that hair shaft diameter had little or no relationship to age, though there were maxima at 10-20, 40-50, and 90-100. The lowest point on the graph appeared at 0-10. Wide variations in diameter were frequently found in the same scalp. In brief the evidence points to the fact that the hair is fine in young children and increases rapidly in size and reaches a maximum diameter or area during the ages 20 to 29. Thereafter it either tapers off gradually or remains fairly constant in size until after 50 at which time it begins to decrease. There are not many data on the change in the index and the results are not entirely conclusive as to the pattern that hair form takes with progressive age.

Our technique has been described in previous reports and will not be repeated here in detail. The hair samples were collected by Steggerda in Yucatan, Mex-

ico. Pure blooded Maya Indians were chosen for this study as it was believed desirable to have the hair from a group of people who were relatively homogeneous from a racial standpoint. Fifty females and 53 males ranging from the ages one to 69 were selected. Approximately 75 hairs from each individual were sectioned and measured. The hair was cut close to the scalp from the same region of the head and the sections in the laboratory were all made 20 mm. from the original cut. Both the males and females were put into ten year age groups and the means and standard deviations with their probable errors were calculated for the area (designated here as square micra divided by 100) and the index. The results are tabulated in Table I.

From Table I and Figure 8 it is apparent that Maya hair is smallest in size in early childhood and increases rapidly in both males and females to a maximum in the 20-29 age group. Thereafter the area tapers off except for an extraordinary rise in the male 50-59 age group. In the majority of cases the females had smaller hair. On the other hand the index drops considerably in progressing from the average age of 5 to that of 15. In the males this diminution continues to the average age of 35 and then remains fairly constant thereafter. The female trend is rather irregular with peaks in the groups 30-39, 50-59, and 60-69, and the index is less than that of the males except in the average ages of 5, 35, 55, and 65. No consistent differences can be found in the variation for either sex in area or in index.

In general it can be said, therefore, that the area of hair sections is least in children under 10 years of age and that it increases rapidly in the succeeding years to reach a maximum size in the ages 20 to 30. These results agree substantially with those of other workers. After thirty years the hairs begin to lessen in size gradually and probably attain the small size of childhood hair in individuals over 60. Other workers have found peaks beyond the 20-29 age group as was found to be true in the males of the present study. We believe, however, that these irregularities are not significant and are merely another indication



CHANGES OF HAIR INDEX AND HAIR SECTION AREA WITH AGE

Figure 8

In males the hair becomes steadily flatter as age increases. In females in this sample trends toward flatter hair with increasing age is not clearly defined. In both sexes the hair tends to become larger during early adulthood and to grow smaller again with age.

TABLE I.—Means and Probable Errors for Area and Index of Cross Sections of Maya Head Hair from Individuals of Various Age Groups.

Age	Area (μ^2/100)		Index	
Group	Males	Females	Males	Females
0-9	27.09 ± 0.35	27.48 ± 0.82	88.49 ± 0.22	88.93 ± 0.33
10-19	37.82 ± 0.32	33.57 ± 0.27	83.61 ± 0.23	82.63 ± 0.23
20-29	40.62 ± 0.47	40.57 ± 0.58	83.47 ± 0.37	80.94 ± 0.44
30-39	39.67 ± 0.35	35.15 ± 0.31	80.61 ± 0.30	84.29 ± 0.25
40-49	33.17 ± 0.42	35.98 ± 0.36	80.62 ± 0.31	77.71 ± 0.35
50-59	39.75 ± 0.44	32.32 ± 0.38	81.50 ± 0.35	83.82 ± 0.34
60-69	32.46 ± 1.13	29.63 ± 0.37	80.06 ± 0.67	85.46 ± 0.38

of the tremendous variability that is encountered in such work. The fact that none of the peaks in the older age groups in any of the studies agree with one another would seem to bear out this point.

The hair tends to flatten out after childhood and from our male series it appears to remain constant from 30 years of age onward. Although our females showed a similar rapid decrease in index in the youngest group, the trend in succeeding ages appears very irregular. Whether this is due to unfortunate sampling is hard to judge; Trotter's[8] figures likewise give an up and down configuration with no consistent pattern. Only further studies of samples taken from the same individual at increasing ages will reveal whether there is actually any consistent correlation between age and the index of head hair.

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Labile Genes

GENES have fallen on evil days. It is hard to be doubted out of existence by the action of a mysterious chromosomal pattern or reaction system. It is hardly less an offense against the dignity of a gene whose main pride has been its crystal-like constancy and purity, to be accused of being labile, and hardly more stable than a lump of sugar melting in a cup of coffee. A bill of particulars almost as bad as this, at least with respect to one gene, seems to have been established in Ernst's admirable and voluminous book.*

Ernst is professor of Botany at the University of Zurich and is well known for his researches and his theory of apogamy. The content of the book is revealed by its subtitle: "Gene mutations as causes of variations in penetrance and expressiveness of a form-anomaly proved by experimental pedigree research on calycanthemous primulas." (Calycanthemy consists of the transformation of the green calyx into a second colored corolla.) From 1923, when he obtained the original calycanthemous ancestor, until the time of publication, Ernst cultivated about 100,000 plants in six successive generations and examined several millions of flowers with respect to the development of their calyces and the relations of this character to the length of the style. Calycanthemy was found to be dependent upon a single dominant gene (*C* or *Cal*), the recessive allele (*c* or *cal*) being responsible for the development of the normal green calyx. *C* was found to be linked to the gene *H* for heterostyly (the dominant allele *H* producing a short style, the recessive *h* a long style). The linkage is not complete, there being about 0.49 per cent crossovers. The frequency of calycanthemous plants can be only partly explained by linkage and crossing-

*ERNST, ALFRED. "Vererbung durch labile Gene," I. Teil. Genmutationen als Ursache von Abaenderungen in Penetranz und Expressivitaet einer Bildungsanomalie, nachgewiesen durch experimentelle Stammbaumforschung an Calycanthemen Primeln. Mit 17 Stammbaumblaettern, 199 Tabellen, 3 Doppeltafeln und 58 Abbidungen im Text. V pp. 567. Zurich 1942.

over. Among the offspring there are many fewer calycanthemous individuals than expected (reduced "penetrance") and many of these display the phenomenon in a lesser degree (reduced "expressiveness"). From his experiments Ernst draws the following conclusion (p. 43): "There is not one but many alleles for calycanthemy which transmit the character in various degrees of penetrance and expressiveness." The *C* gene is conceived to be constantly in a state of mutation, the mutations always weakening the intensity of effect and thus approaching the recessive allele *c*, for the normal green calyx.

The author supports his hypothesis by detailed pedigrees of 17 plant families, employing clear and practical symbols and modern statistical methods. The phenomena here revealed can be explained by the theory that the *C* gene is labile and is characterized by the existence of a series of alleles from C^{v} (complete calcyanthemy) to $C^{l\text{-}n}$ (almost normal green calyx) (p. 342). It is noteworthy that not only different individuals of a single family but also different flowers of a single plant may transmit calycanthemy in different degrees of penetrance and expressiveness (p. 458). Even in a family characterized by a very high degree of penetrance and expressiveness, mutations in the direction of green calyx can cumulate rapidly enough to extinguish calycanthemy often in only two generations (p. 217).

As was indicated by the title, the author reports in this volume only the first part of his investigations, the crossing experiments and the pedigrees. We may expect a future volume dealing with the results of cytological investigations.

In the present volume the author limits himself almost exclusively to the experimental record. He neither discusses the theoretical consequences which may be drawn from his results nor the relations of his observations to other investigations and to current theories. There seem to be connections and similarities between Goldschmidt's *Lymantria* experiments which established his physiological theory of genetics and the facts revealed by Ernst, but not even the name of this author appears in the index of literature. Some of the broader consequences of his observations do come in for brief mention. Thus on p. 13 the author discusses the question: "whether there are some among the many human anomalies and hereditary diseases which in contrast to the present assumptions may show a decrease or elimination within the affected families under certain circumstances."

Looking at this thick and abundantly illustrated volume devoted to a purely theoretical subject remote from war and defense, we have reason to envy the small country which managed to remain at peace in such tumultuous times. And there is another consideration obvious to the reviewer: what would have been the fate of Gregor Mendel's work which the author quotes in his introduction, if Mendel had given his findings in such a book, instead of a slender pamphlet of some 40 pages?

Hugo Iltis

Mendel Museum,
Fredericksburg, Va.

THE BIO-SYSTEMATIC NO-MAN'S-LAND

A Review

A CONCEPT which is becoming increasingly prevalent in genetics is the notion that natural selection operates not upon the individual species as such but upon the entire biological spectrum of any particular habitat. More than one able geneticist has started out to do exact quantitative field work on micro-evolution only to find that before the investigation could get down to fundamentals exact cyto-genetic techniques would have to be accompanied by equally precise ecological analysis. There is therefore an urgent need for books which make cytogenetics understandable (and palatable) to ecologists and plant geographers, and *vice versa*.

Professor Cain has attempted even more. His book,* in his own words, is "an effort to survey the related fields of science for concepts and working methods which are useful in an interpretation of the phenomena of distribution." This is quite an order even for as able and energetic a student as Cain, and he acknowledges as much in a very frank introduction. While he has surveyed the entire field of biology in his search for foundation stones, he has drawn most heavily upon genetics. A survey of authorities cited in the index (an excellent one, by the way) shows that nearly half of his detailed discussion falls in the field of cytogenetics and less than a quarter in ecology, with geography, palaeontology and pure taxonomy bringing up the rear and in that order. It has therefore been a task doubly difficult. Not only was most of the literature outside of Cain's own field of ecology but much of it was written by people who were themselves quite unacquainted with plant geography. Hence he has had to scan a great variety of material for implications which were beyond the field of view of the authors themselves. Anyone who is willing to tackle such a program merits the thanks of workers in all these fields. The book will focus attention upon a number of fundamental problems and will serve to reveal to more than one scholar unsuspected implications in his own work. It is, however, an attempt more noteworthy for its breadth of view than for its integration, a situation more or less inevitable in any such pioneer undertaking. A great deal of interesting material has been brought together to construct the foundations of plant geography, but (as the author is apparently aware) the actual foundations have yet to be built.

Palaeogenetics

To plant geneticists one of the most interesting parts of the book is the chapter on "The history of cenozoic vegetation in North America." It is a summary of the remarkable work carried on in the last two decades principally by R. W. Chaney, H. L. Mason, and their associates. Fragmentary and incomplete as the fossil record must inevitably be, enough is now known in this area to give a reasonably coherent picture of developments during the Tertiary, the Pleistocene, and the Recent periods. Chaney's broad view of the developments and migrations of such vegetational units as the redwood forests forms a fitting prelude for Mason's precise analysis of evolution in certain West Coast pines from the Pliocene to the present. The latter work is apparently unknown to most geneticists, partly because Mason is too busy with administrative and teaching duties to publish more than an occasional brief report. This is unfortunate because it is to all intents and purposes a series of population analyses of a developing group of species through a series of geological eras. Seen as a whole it forms a beautifully precise demonstration of the effects

*Foundations of Plant Geography, by STANLEY A. CAIN. 556 pp. plus xiv. Harper & Bros. 1944.

of isolation, when islands were cut off, and of the effects of invasion when they were again tied to the mainland.

A further chapter summarizes the work on pollen peat analysis which both in Europe and North America has given us an outline of post-glacial changes in the flora of the northern temperate zone. Here as elsewhere in the book the author has not only redrawn the investigators' original figures to good advantage but he has frequently brought scattered data together from various papers into one easily-grasped diagram.

One of Cain's best organized chapters deals with criteria for the indication of centers of origin. C. C. Adam's pioneer attempts in this field have been integrated with recent work by Hulten and examined in the light of modern genetics. The author echoes the opinions of our leading American plant geographer, H. M. Raup, and concludes that biogeography and ecology carry "a heavy burden of hypothesis and assumption which has resulted from an overemployment of deductive reasoning. What is most needed in these fields is a complete return to inductive reasoning." He feels that students of this subject are so permeated with dogma that they "can distinguish fact from fiction only with difficulty." With these conclusions the reviewer is in complete agrement. Geneticists should find this chapter a challenge to setting up programs of investigation which might contribute exact and significant data on otherwise obscure problems.

Cytogenetics and Taxonomy

In the field recently christened "Biosystematics" (which forms a sort of no-man's land between Taxonomy and Genetics), the last two decades have shown that the details of evolution and the patterns of speciation vary from genus to genus and from family to family. Much of this literature is pertinent to the author's purpose and he has surveyed an impressive number of papers, giving more than passing notice to the effects of introgressive hybridization and devoting an entire section of the book to "The Significance of Polyploidy in Plant Geography." Other variations of evolutionary patterns have been rather completely neglected. Apomixis, for instance is not mentioned, though in the opinion of the reviewer it is a phenomenon of even greater significance for plant geography.

All of which brings us to the most curious omission in the book, but one which marks the author as merely in line with American geneticists rather than with English ecologists. At practically the same time, but by very different avenues of thought, two of the world's outstanding biologists hit upon the peculiar importance of population size as a factor in evolution. One was the American geneticist, Sewall Wright; the other the English ecologist, Charles Elton. The former phrased his discussion in terms so mathematical that all geneticists were impressed (whether they understood them or not) but ecologists were left completely baffled. Elton, on the other hand, employed qualitative terms of such simplicity that his work was ignored by many American ecologists* as well as by most geneticists. Few of them looked carefully enough into Elton's short contributions to discover that the simple words set forth important facts; that the thinking behind them was exact and was accompanied by a peerless understanding of evolutionary phenomena.

There have developed therefore two independent sets of papers concerning population sizes. Some biologists read Elton's little book. Others read Sewall Wright (most of them indirectly *via* Dobzhansky whose immortality is secure, if for no other reason, as the man who made Sewall Wright intelligible to ordinary, grade-A biologists). A critical examination of these two sets of contributions is long overdue. Since Cain is himself an ecologist we might have expected it from him, but though he has gone through the difficult

*Just in passing it might be mentioned that Cain's book will serve as a harmless guide to the incredible terminology of one school of American ecology.

Wrightian literature, the works of Charles Elton are neither mentioned nor cited. However, it is a good deal to ask. An author who has already plowed through a bibliography of 720 titles is scarcely to be blamed for not taking on 10 or 20 more. Let us hope that when some equally daring geneticist essays the reciprocal job of reviewing plant geography for its significance to genetics, that he will produce a volume as provocative as this one.

EDGAR ANDERSON
Missouri Botanical Garden, and
Washington University

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THE OCCURRENCE OF A "SILVER" TYPE MUSKRAT

COAT color has been one of the most frequently studied characteristics of many animals. It is beyond the scope of this report to attempt a complete review of this voluminous literature on pigmentation and pigment modification. Since this note reports a possible genetic modification of wild type color in the muskrat, it seems logical to cite a few recent reports on color variations in mammals. Smith[3] and Gregory[1] have noted the occurrence of albinism in the opossum and the raccoon respectively. However, the most important recent color variations in fur bearing animals are the platinum mutation in the Norwegian Silver fox and the platinum, blond, and silver mutations in the mink. The mutant type fox has been discussed by Mohr and Tuff[2] and the three mink mutations by Smith, Whitaker, Davids, and Noble.[4] In both of these species the mutant types are being exploited commercially.

In view of the success which has attended the development of the mutant type mink and fox it would seem worth giving attention to like variations in the muskrat. Although muskrat pelts are among the less valuable furs, they are still in the aggregate the most important source of fur in the United States. Far greater numbers of muskrats are taken in the United States each year than any other fur bearing animal. Muskrats are widely distributed over the country and in most of the major fur producing states this species is the leading fur animal in point of number and total value.

Muskrats trapped in different parts of the country vary considerably in size, heaviness of fur, and color. The type most commonly taken in Louisiana is referred to as dark or black. However, the actual outward color of the pelt is brown and not black. It is called "black" or "dark" because it is a deeper or darker shade of brown than is found in some other sections. There is, in fact, considerable variation in the density of color of rats trapped at different points within the state.

The individual to be described in this report was trapped during the 1943-1944 season. It was taken in the coastal marsh land of Cameron Parish (county) in the extreme southwest part of Louisiana. The animal attracted attention when the pelt came into possession of a local fur buyer. It is unfortunate that no study could be made of the entire animal. Thus, no statement may be made as to the pigmentation of the eyes of the animal.

The pelt appeared quite normal in all respects except color. The fineness of the under-fur was apparently normal, and the number and distribution of the guard hairs was normal. However, the color was distinctly "silver" on a dark or black background. Both the silvering and the dark or black general color were considered unusual. When placed in direct sunlight the "silver" appearance was most pronounced. Examination of the individual guard hairs revealed that there were white and black fibers. The black fibers were predominant, giving the animal the basic black background color. The white fibers were sufficiently numerous and evenly distributed over the back and sides to produce the "silver" effect. This uniformity in distribution of the white fibers was marred by the presence of two black spots, areas in

which the white hairs were almost completely lacking. One of these areas was located at the base of the skull and the other just anterior to the tail. These two spots were almost completely black. The color paled generally from the back toward the belly of the animal.

Close examination of the individual guard hairs showed them to be uniform in pigmentation over their entire length. Neither the white nor the black hairs were tipped or banded.

In the underfur there was a typical shading of color from light to dark in moving from the side to the belly and from the side to the back. Of particular interest was the fact that several darker areas in underfur were noted. These were located on the sides and did not coincide with the two areas of black guard hairs noted above. The color of the underfur in the areas of dark guard hairs appeared to be the same as that in the surrounding silvered area. The variations in the depth of underfur color were generally very indistinct and would hardly be sufficient to justify classing the animal as pure spotted.

From this single individual it is not possible to make any suggestions as to any possible hereditary basis of the condition. It is considered important because of its similarity to color variations, in other animals, which have proved to be hereditary. If similar variations occur in the muskrat the capture and breeding of such individuals might result in the development of silver strains of considerable economic importance.

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J. Lane Fletcher
John McNeese Jr. College,
Louisiana State University

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GENE MECHANISMS

THE genes produce their remarkable results not by magic but through the use of normal developmental mechanisms which are subject to investigation. The exceedingly important role of Rh antibodies, in producing erythroblastosis and local as well as general anoxemia, is receiving much attention. The local effects in the central nervous system are particularly important as they lead to progressive deterioration in infancy, if not earlier in fetal life. It is suggested that a possible explanation of the local effects is the persistance of mesenchymal blood islands in the meninges beyond their normal period of activity in 10 to 20 mm. embryos. Such islands, when the blood pressures rise with increased cardiac efficiency, could well form the basis of arterio-venous anastomoses which would then short circuit portions of the central nervous system. If the anastomosis occurred between the carotid and cavernous sinus at the brain base the whole brain would undergo degenerative atrophy. Such anastomoses are known to occur in the gene mechanism of the creeper fowl and could logically develop in many places. Some such mechanism may underlie the anencephalic monster in which cranial nerves and spinal cord may be fully preserved while the brain atrophies. We have one infant in which syndactyly and brachydactyly were correlated with a normal heart too small for the body it supplied.

Investigation of the vascular portions of gene mechanisms could pay large dividends in results.

John S. Sinclair
University of Texas
Department of Anatomy
Galveston

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